Though likely to be of use to geologists and botanists, the treatment is sufficiently popular to be intelligible to the general reader. The floras of the successive geological formations are treated of in turn, from the oldest rocks down to comparatively recent times. The two longest chapters in the book are devoted to the vegetation of the Devonian and Carboniferous ages respectively, much of the matter here traversed having formed the subject of numerous scientific memoirs by the author. In the body of the work, accounts of the morphology and minute anatomy of the various plant-remains are given, with speculations as to their affinities, and in many cases restorations are attempted, illustrated by figures. The more special details as to classification, &c., are wisely placed in small type as a series of notes at the end of each chapter. The last chapter in the book consists of an interesting essay on the general laws of origin and migrations of plants. Many of the woodcuts leave much to be desired, more especially those dealing with histological subjects. These are, for the most part, scrappy and insufficiently described, and convey little to the mind. Comparisons between fossil remains and recent plants are often rendered valueless by strange inaccuracies as to the morphological value of the parts so compared. Thus the leaves of *Marsilea* (pp. 60 and 67) are described as being in whorls and cuneate in form, and in *Azolla* and *Salvinia* the leaves are "frondose and more or less pinnate in their arrangement." *Spenophyllum*, which possesses wedge-shaped leaves arranged in verticels on the stem, is set down as of probable Rhizocarpian affinity, on this mistaken comparison between its *leaves* and the *leaflets* of *Marsilea!* Much confusion also arises from a careless use of the terms sporocarp, sporangium, macro- and micro-spore, antheridium, &c., in connection with certain small bodies found in the Erian and Carboniferous beds, and conceived by the author to be the reproductive bodies of a rich, then-existing Rhizocarpian flora. Though there are many points in which palæobotanists may not be at one with the author—such as the reference of so many Palæozoic forms to Rhizocarps—the volume will be of service, especially to those to whom the larger treatises are not available.

LETTERS TO THE EDITOR.

[*The* **Editor** *does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Prophetic Germs.

I HAVE but just returned from abroad, and have hastened to read the number of NATURE for August 30. I find that the Duke of Argyll in his letter of that date makes some remarks which call for a few words from me. The Duke is not, it appears, prepared to defend the theory that the electric organ of *Raia radiata* is a "prophetic germ." He refers me to the paper of Prof. Ewart on this subject, whose opinion he quotes and accepts. I am not sure how far Prof. Ewart himself had considered the significance of the view which he put forward in regard to the nature of the rudimentary electric organs of skates; but I do not hesitate to say that there are no facts which have been made known at present, either by earlier observers or by Prof. Ewart, with regard to the electric organ of skates, which necessitate such a theory of prophetic germs as that imagined by the Duke of Argyll, or which can be shown to be inconsistent with the doctrine of progressive development by the natural selection of fortuitous congenital variations. If the Duke of Argyll will point out such facts, he will have made a contribution of some value towards the understanding of the laws of organic evolution.

In a subsequent portion of his letter the Duke of Argyll states: "If Prof. Ray Lankester will explain how 'natural selection' can act upon 'congenital variations,' which he calls 'non-significant'—*i.e.* which are not yet of any actual use—and if he will explain how this action can afford 'the single and sufficient theory of the origin' of (as yet) useless variations, he will have accomplished a great triumph in logic and philosophy."

I am unwilling to entertain the notion that the Duke of Argyll has intentionally constructed the above sentence by garbled quotations from my previous letter in order to produce the false impression that I have maintained such a view as to the action of natural selection. At the same time, I will observe that the method of discussion adopted by the Duke—namely, that of half-quoting the opinion which he attributes to an opponent and desires to render illogical in the judgment of others—is, to say the least of it, objectionable. It becomes easy when this method of partial re-statement is adopted for the disputant to insert words of his own mixed with the words of his opponent, and thus to misrepresent the latter's statement by unconsciously fabricating what the poet has condemned as the worst of fabrications—namely, one which is half a truth.

The point of the sentence which I have above quoted from the Duke of Argyll's letter depends upon the unwarrantable introduction on his part after the quotation of the word "non-significant" of certain words in explanation of that word. The Duke is kind enough to say that by "non-significant" I mean "which are not yet of any actual use." I have not had any private communications with the Duke of Argyll upon this matter, and am at a loss to understand how he should have come to think that he knows that this was what I meant by the word "non-significant." By whatever process he arrived at that conclusion I regret to have to say that it is absolutely erroneous. My meaning was nothing of the kind, and I was under the impression that I had stated with sufficient clearness what my meaning was. It appears that I did not state it clearly enough for all readers. I called the congenital variation which survives in the struggle for existence "non-significant" in regard to its origin and not in regard to its survival. It was, I think, clear to most readers that I was distinguishing between the Lamarckian theory of variation as due to the transmission of parental acquired characters and the Darwinian theory of variation as due to a "shaking up" of the germ-plasma at the union of egg-cell and sperm-cell. The variation—that is, the departure of a young animal or plant from the normal character of the species—would be, if it could be traced to the transmission from a parent of a character acquired by that parent in adaptation to the environment, significant; that is to say, it would have significance for the adjustment of the species in its very origin in the parent. On the other hand, the thousand and one slight or considerable departures from the mean specific form which occur in every possible direction in a brood of young fish or other organisms are "non-significant." They are due to a long-precedent disturbance of the germ-plasma when the form of the organism was undeveloped. No possible reaction of adjustment can be imagined which could produce adaptation in the structure of an animal or plant developed from a germ, if it be a proviso that such adaptation is to have relation to a physical cause of disturbance which once acted upon the germ whilst the adaptational results are to come into effective existence in the developed product of the germ. Hence I am led to speak of congenital variations as "non-significant" in relation to the disturbing causes which produce them.

The proposition that congenital variations are selected when they are not yet of any actual use is an absurdity which the Duke of Argyll had no justification whatever for suggesting as likely to be defended by me, and one which he arrives at by misrepresenting the meaning of the adjective "non-significant." As a matter of course, some one combination of congenital variations is "significant" in the sense which the Duke of Argyll chooses to give to that word—a sense in which I do not employ it: some one combination of congenital variations in each generation survives because it is "significant" in the sense of being useful. It is a common fallacy to suppose that natural selection is only operative in producing *new* species; on the contrary, it is never in abeyance, but is equally as active in maintaining an existing form as in producing a new one.

With regard to the origin of useless variations and the general question of uselessness, it is not to be expected that your columns should be given over to an exposition of the common-places of Darwinism. It is to be noted, firstly, that we have no right to conclude that a structure is useless to the organism in which it occurs because the Duke of Argyll is unable to see in what way

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it is useful; secondly, we have established the great principle of "correlation of growth," which is a brief way of stating that in organisms there is such an intricate binding together of the mechanism that when one part varies other parts vary concomitantly—so that a useful variation of the beak or eyelid of a bird (for example) may *necessitate* a concomitant and perfectly useless variation in the toe-nails or the tail-feathers, thirdly, useless structures undoubtedly exist owing to the potency of heredity, which is of such strength that long after a structure has ceased to be a matter of selection it is transmitted from generation to generation, though dwindled in size and more or less imperfect in structure.

I think there will be no difficulty by reference to one or other of the three considerations above stated in disposing of cases of so-called "uselessness," or "prophetic" functionless organs "on the way to use," which the Duke of Argyll may find to be stumbling-blocks in the way of his faith in Darwin, if he will submit them one by one for pulverization, though I am afraid the process will not interest your readers.

September 21. E. RAY LANKESTER.

A Shadow and Halo.

A FEW evenings ago, whilst walking down a sloping pasture, with the moon shining brightly at an altitude of about 20° behind me, and with no visible dew nor fog, yet with heavy dew on the grass, I noticed that the shadow of my head and shoulders was very sharply defined, but that it was surrounded by a halo of light, and that this halo or nimbus increased in brightness as my shadow was lengthened out because of the increasing slope; and not only was the brightness increased, but it extended now to my hips. That this was due to the greater depth of moist air through which the moon's light passed, by reason of the increase of the slope, I think was proved by the fact that in the neighbourhood of a high hedge, which would to some extent alter the conditions, this halo nearly wholly disappeared. At one time I thought that my eyes were deceiving me concerning this appearance, the contrast of the dark shadow with the surrounding brightly illuminated grass giving rise to the appearance above mentioned, but, by holding up my hand so as to cut off the view of the shadow, I still saw the brighter light which surrounded it, and this brightness still increased or decreased in intensity as the slope on which I took up my position was greater or less. There was no casting of a shadow on a fog-bank, as there was no fog at all, but rather the air was particularly clear. I noticed this phenomenon three nights in succession. I shall be glad to know if any other amongst your readers has noticed this occurrence, and will explain it. E. W. P.

Tamworth, September 29.

Sonorous Sands.

REFERRING to Mr. Carus-Wilson's letter recording the supposed discovery of musical sand in Dorsetshire, I may mention that about two years ago the late Admiral E. J. Bedford sent me three boxes of musical sand, one of them being labelled, "Musical sand; Studland Bay, Dorset, 1885; sonorous when collected." I am not aware whether Admiral Bedford himself discovered the sonorous properties of this sand, but it is clear that he was well acquainted with both the sand and its character in 1885. A. R. HUNT.

Torquay, September 27.

THE REPORT OF THE KRAKATÃO[1] COMMITTEE OF THE ROYAL SOCIETY.

I.

AFTER an interval which has been prolonged partly by the unexpected continuance of the subsequent atmospheric phenomena, and partly through other circumstances incidental to publishing, the Report on the great eruption of the volcanic island of Krakatão in August 1883 is now before the world.

Every Committee is bound to issue a Report of some kind, but it rarely falls to the lot of a Committee to deal with anything at once so stupendous in its character and far-reaching in its consequences as the eruption which not only figuratively, but literally, vibrated through the world on August 27, 1883.

We, in these islands, may boast of our Essex earthquakes, and of the frequent little tremors to which a certain district in Perthshire is subject; but few of us, or our immediate neighbours, can, from our local experience, form the faintest conception of the terrific subterranean powers which ordinarily manifested themselves in the volcanic region of which Krakatão may be fitly termed the *focus*.

The first accounts which reached us by telegram, inaccurate though they were bound to be as regarded details, were scarcely exaggerations in point of magnitude; and, indeed, the cataclysm in this case rose superior to all artificial modes of transmission, by announcing the very date and hour, if not minute, of its culminating explosion through a series of air-waves, which recorded themselves no less than four times on every automatically recording barometer throughout the world.

Three other distinct and abnormal phenomena were: (1) the immense distance to which the sound-waves were propagated (altogether transcending anything hitherto on record; (2) the immense local height, destructive power, and subsequent wide diffusion of the accompanying sea-waves, which in this case were *not*, as is usually the case, due to earthquake action; (3) the simultaneous occurrence in the Javan and Indian area, and subsequently rapid extension, first round the equatorial zone, and, finally, to the whole world, of a most remarkable group of optical phenomena, including coloured suns, lurid and prolonged glows at twilight, large coronæ round the sun and moon, and a peculiar cirriform haze which was evidently connected in some way with these and the eruption.

It was plain, in the face of these preliminary facts, that the collection and discussion of such a grand series of exceptional phenomena gratuitously evolved out of Nature's own laboratory, could not fail to be of service to science, and that while the more local features and practical results of the episode might be left to the Dutch Government, to whom the district belonged, its attendant and subsequent phenomena deserved permanent record in the pages of scientific history.

On this basis, a Committee of the Royal Society was appointed on January 17, 1884, in the following terms:—

"That a Committee, to consist of Sir F. Evans, Prof. Judd, Mr. Norman Lockyer, Mr. R. H. Scott, General Strachey, and Mr. G. J. Symons, with power to add to their number, be appointed to collect the various accounts of the volcanic eruption at Krakatão and attendant phenomena in such form as shall best provide for their preservation and promote their usefulness."

The subsequent expansion of the Committee by co-operation of additional members, and the substitution of one—Captain Wharton—in consequence of the death of Sir Frederick Evans, is detailed in the preface.

The main object of the Committee was thus to collect facts and reduce them into a systematic and useful form. While this has been its primary object, it has been thought advisable to enlarge upon the original basis of the Report, and, while giving a *résumé* of all the leading opinions, especially those relating to the debated question of the relation of the optical phenomena to the eruption, to enter at some length into a discussion of the facts thus systematized. Though it is hardly to be expected that everybody will agree with the deductions arrived at by each author, and though it has been impossible to avoid omissions in a work embracing, in its latter sections, observations extending over three years, and a literature of its own, the main facts have not only been recorded, but, as the Chairman, Mr. G. J. Symons, says, can be readily verified.

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